PLANS FOR DEVELOPMENT UNDER THE PURNELL ACT

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Next to the inauguration of the American system of experiment stations the passage of the Purnell Act is the most momentous step in the progress of agricultural investigation in any country. The circumstances which led to it at this time will naturally increase the expectations of results to flow from it. But these things need cause no concern if faced with full realization of wheat is now implied in the way of proper organization, administration, personnel, and general conditions. That, I take it, is what we are here for.

Since the purpose of the Act is to build upon what already exists, a brief survey may be profitable to show clearly the background against which the Purnell Act is projected.

The experiment stations embraced in the National System reported total revenues for the fiscal year 1924 aggregating something over $10,000,000. There were direct appropriations from the States amounting to more than $6,000,000; the Federal Government contributed $1,440,000, and the other revenues were from fees, sales, and miscellaneous sources. This sum is equivalent to the interest on an endowment fund of $200,000,000 at 5 per cent.

There were listed as in progress at these stations last year a total of 5,538 projects, a tremendous number when we consider what effectiveness in so many diverse efforts implies. It is equivalent to an average of approximately 110 projects per station; and eliminating those of purely administrative or regulatory character, the average amount available was something over $1,800 per project. On the basis of the amount of funds reported as spent for research, however, ($7,272,814) the average per project was about

$1,325, which will be recognized as too low for good results.

The personnel of the stations includes about 1,400 technical workers. On a liberal estimate of 1,000 independent workers or project leaders, there would be an average of five projects per leader. This would mean that some had more than this, since many had less. The question whether this is too many may depend somewhat on circumstances--the kind of work and the conditions under which it is done. But there is a tendency of many workers to scatter their efforts over too many projects, not to go deep enough to get the basic facts, and not to put enough of themselves into any single experiment to be constructive. Diffuseness is one of the evils that still needs to be guarded against.

Even a cursory examination of the projects under way shows what a remarkably extensive range they cover, and the innumerable phases that are included. Hardly a subject in the field of agricultural production can be conceived of that is not covered in some form by a project at one or more of the stations. For example, there are 1817 projects in field crops--the production and cultural side, including 172 on corn, 164 on wheat, 162 on potatoes, etc. Horticulture follows with 952 projects, including no less than 115 on the single subject of apples. Forty-nine stations (all but one) have a total of 639 projects on soils and soil fertility. These range all the way from a study of the soil solution as affected by various circumstances and treatments, through comparisons of different fertilizing materials, to the long-time rotation and fertilizer experiments. Animal nutrition and dairying aggregate more than a thousand projects, with 85 on the principles of nutrition, and in the neighborhood of 600 feeding and management experiments with horses, cattle, sheep, swine, and poultry. On poultry alone there is a total of 234 projects, in which no less than 35 of the stations are involved; while on hog feeding there are 195 projects at 39 sta-

tions. The subject of dairy husbandry and dairy manufactures embraces 320 separate projects, carried on at 45 stations.

For studying and combating the enemies of production there are 479 projects in plant pathology, 493 in entomology, and 177 in veterinary medicine. Rural engineering shows a total of 163 projects, and rural economics 198 projects. Work in the latter line (economics) is reported under way at 31 stations; and there are 130 projects related to the field of home economics at 33 stations.

These facts show the great magnitude of the station enterprise in its present form--a ten-million dollar undertaking, employing 2350 persons for at least part of their time, using 75,000 acres of land, and having under way nearly 5,500 research projects. Many of these projects deal with the same or quite similar subjects, so that apparently the need in the field being covered is not always for more projects but for a better organization of those already existing.

It will be realized that these projects represent widely varying degrees of intensity. For example, a fairly careful estimate indicates that from a quarter to a third of the work in progress is of quite elementary routine character, considered from the standpoint of the present time and the stage of experimentation, involving little that is new or more effectual. It is of the cut- and-try order, following conventional methods, going over ground previously covered, but not effective in settling questions long under discussion. Such work deals to a large extent with comparisons, these comparisons being between complexes embracing a variety of factors in varying proportions, often little understood. It is not to be condemned as a whole, by any means, but to considerable extent it has not shown itself adequate. It aims

to get practical answers or the basis for practical advice, but at most it usually supplies only certain broad suggestions and ephemeral results, limited in their application.

Age is manifestly not a proper criterion in judging the merits of experimental effort, and there is a vast difference between a live, progressive experiment, and one which has become mainly an exhibit. Some of the rotation and long-time fertilizer experiments have not been published upon for a decade or more, nor the data studied sufficiently to show where the work is leading or whether it is giving new information.

There has been a vast amount of repetition in experimental work, as everyone familiar with the subject realizes. This was to be expected in the early years, but it has continued on too large a scale, and the duplication too rarely constitutes replication of a kind to make results comparable. In many instances we have been undertaking to answer too many questions by an experiment. This is true in feeding trials where a wide variety of compounds and methods of feeding are compared in a single experiment, and in field trials which attack the broad problem of soil fertility for a particular soil or cropping system. Such experiments deal with comparisons between different combinations, with such a large number of variations as to limit the number of checks and replications and make conclusions difficult. The aim is the laudable one to get information which is practical, but the work fails to establish permanent values, with the result that conditions often change before the conclusions are hardly out, and so further repetition is necessary.

The simple method of making mass determinations of various kinds or continuing routine experiments over an indefinite period has given volumes of figures, but in the final analysis has been disappointing in the conclusions.

This is not the method of science to-day, and knowledge is not being advanced in that manner. There is still too much of "letting the data speak for themselves," as contrasted with framing the procedure to give data which can be intelligently interpreted. The prime object of investigation, of course, is to accumulate knowledge--not data in notebooks but digested facts, correlated if possible with other existing facts and information.

It is important to bring to a more conclusive stage the experiments on many common subjects which have long been under way, but to do this will often call for more decisive measures, or at least more discriminating and energetic action. Unproductive effort should naturally be eliminated as rapidly as revision can be safely made.

The reason for calling such frank attention to these matters at this time is not to condemn natural mistakes but to avoid perpetuating them under the new fund. Presumably it will be planned to use some of this new fund on projects already under way, but my feeling is that this should only be done after a thorough examination of such projects as to their plan, status, and the promise they hold. The Purnell Fund ought to stand for sound investigation in accord with modern conceptions.

In strengthening the experimental work emphasis may now be laid on greater definiteness of purpose, with less experimenting in the dark. The investigator needs to know what he is going after. This implies intimate familiarity with the status of the subject to be investigated; a knowledge of the nature of the problem, developed out of previous study; and on the basis of this reasoned, constructive investigation. We already have a pretty broad general background in the older subjects of investigation. New or further work may be expected to deal with building upon this, a further interpretation of existing data, or

filling in the gaps in what needs to be known. This is where study is needed in projecting new efforts. Unless a worker has fully digested what has been done on a subject and has conceived some definite ideas for steps which will advance it, it may well be questioned whether he is in position independently to undertake an investigation.

A project is more than the plan and specifications; it is an expression of the need and feasibility, an evidence not only of what would be desirable but what may be expected. Results can not be guaranteed, but assurance can be given that everything feasible is being done to warrant expectations. Plans can not be crystalized and stated fully in advance; they will need to be modified on the basis of results as they accrue. In making experiments or investigations we take a risk as to the outcome, but we can act in faith.

Duclaux said that science progresses above all by change in the point of view. This is equally true of progress in experiment and investigation. It is only as new ideas are evolved that there is the basis for progress. It is only by the arrangement and close study of the data as they are secured that we have any right to a point of view, or any proper basis for changing the accepted one. The aim of the investigator must be to bring his work to a point where he can <u>form a critical judgment</u>. If he can not do this he can not make the interpretation and draw conclusions. It is a serious defect when neither positive nor negative results can be detected, especially if the line of attack isn't modified. Hard working investigators sometimes seem to lack ability to bring their work to a point where they can form a critical judgment to guide them. This, of course, limits a worker's creative product and the extent to which he can be left to himself.

We need in increasing degree workers of broad training, with vision and

ability to put originality into their tasks. Intellectual curiosity and initiative are impelling motives in investigation. Initiative is the combined expression of zeal, alertness, and ingenuity; the readiness to act on a new idea and the urge to carry forward. Without it there is routine, lack of hard thinking, failure to recognize new suggestions when the data furnish them, and to detect gaps which need to be filled in to make the sequence complete.

There are many instances of missing important discoveries because of failure to notice discrepancies and attempt to account for them. Apperception--the relating of new ideas to familiar ones--and the reverse of overlooking things that are not understood or can not be fitted into what is known, are expressions of the extent to which alertness and thinking enter into investigation--the difference between a constant attitude of inquiry and following a mechanical procedure. As has been said, with the majority of people inspiration comes as a result of perspiration.

I am sure I will not be misunderstood in what I have said, or be thought unfairly critical. Certainly I will not be charged with failure to appreciate the vast amount of high-grade, constructive investigation that is under way. But it will be recognized that most every station has workers of varying degrees of ability to plan and to execute productive work independently. Some call for more attention than others. It is a function of administration to detect the need and supply this attention. Judgment as to the competence of a worker does not wait on his final results; it is supplied in some measure in the way he conceives and outlines his projects. Often he can be helped at that stage. If not, he may be too speculative a risk to be entrusted with an expensive piece of work.

How then shall the stations secure the kind of help needed, when it is limited and expansion is common to all of them?

(1) By appreciating the exceptional character of ability essential to productive research, and making the salary, the conditions, and the outlook fit the specification. This now is done to some extent, but some executives appear afraid to carry it out--afraid of public criticism and fearful of the effect of inequalities in the faculty. Such a thing as a flat salary scale is incompatible with growth and a high efficiency in research. The authorities and the public need to understand that there are other far more effectual means of wasting money than by paying high salaries to the unusual investigator.

(2) By fostering the idea that ability in research will be rewarded, and that rank and salary are attainable in that field as well as in administrative positions.

(3) By utilizing to the fullest advantage those of proven ability on the staff--by recognizing the man of creative ability in research as too rare and precious to be sidetracked into other lines, to the disadvantage of his research.

(4) By placing such exceptional specialists in positions of leadership, giving them adequate assistance, encouraging cooperation, and through them training and instilling into others the standards and methods of research.

(5) By setting high standards of requirements for new appointees. Many graduate students are going out every year to take positions. Others will be induced to meet the requirements when they are set.

(6) By protecting the workers from themselves, holding the scope and the number of projects within the range of their accomplishment.

Is the necessary encouragement being given to attract workers to the field and to hold those of exceptional ability? A survey of the latest data available shows that the salaries of persons ranked as department heads, leaders of

separate lines, and independent workers, range all the way from $2,400 to $6,000. Both of these extremes are naturally exceptional. In three States the range is from $2,400 to $2,750. For the majority of the stations the range is from $3,000 to $4,500.

The maximum salary paid such workers is $3,500 or less in 7 States, and in 30 States it does not exceed $4,000, while in 8 others it is within $4,500. In only 9 states is the maximum for department heads of independent workers $5,000 or over, and in those States this maximum is participated in by only a relative few.

The salaries of directors are naturally more liberal. They range all the way from $3,200 to $10,000, the position in the latter cases being combined with that of dean. Where the directorship is separate from that of dean, however, it rarely amounts to as much as $5,000, and in the majority of cases is between $4,000 and $4,500. The margin between the salaries of department heads and those of deans or directors is often very wide, which leads to the feeling of many workers that a good salary can be attained only through giving up research and taking over an administrative position. In two geographical sections of the country the difference between the maximum for directors and for department heads amounts to $4,500; in one other section it is $2,500, in two others $1,500, and in one it is only $500 to $1,000. In many instances high administrative salaries are not accompanied by a relatively high salary scale for workers.

On the whole, it can not be claimed that the experiment stations are extravagant in the matter of salaries, and considering the large number of workers, these positions hold out relatively few large prizes. Despite this, however, the field and the opportunity have attracted and retained many special-

ists whose work has been of such character as to give them national and even international reputation.

Coming now to the provisions of the Purnell Act, it may be mentioned in passing that it authorizes annual appropriations, and hence is limited to the fiscal year. These appropriations are made to the States but are directed to be paid "to the treasurer or other officer duly appointed by the governing boards of such agricultural experiment stations to receive the same," and such officers are to render an account of their use. The responsibility for the fund therefore rests with the institution, which can not absolve itself of that responsibility for any part of it. It can not give away or assign a part of it to another institution, and there would seem to be no authority to apply it outside its own organization except through cooperation. Individuals located elsewhere may be added to the staff for the purpose of carrying out investigations under the general direction of the station, and staff members may be sent temporarily to other localities when advantageous to the work, for the undoubted purpose should be to get the necessary work done in the most advantageous way; but it should be upon organized projects of the station, and the officers of the latter can not delegate responsibility or relinquish interest in relation to any such features.

The Act is a rural measure, applying to such matters as pertain to the agricultural industry and rural life. It is not designed for essentially urban problems or conditions of life; it is primarily for agriculture and the people living in the country.

As the appropriation is a separate one, a separate account upon it will be necessary; and the account for the entire fund will naturally be kept at the station headquarters, with the usual approval of vouchers by the director

and the provision for an annual financial report covering expenditures for all activities under the fund. Sales or other proceeds arising from work under the Act may be combined with those from other similar sources.

Any investigations or experiments appropriate to the Hatch Act will be appropriate under the new Act, and it is likely that it will be desired to use the latter to some extent in strengthening lines of work in which the stations are now engaged. Comparatively small additions to the funds now available for some well established projects may hasten conclusions or possibly mark the difference between comparative failure and definite success. Wise administration of the Purnell Act will therefore call for consideration of the claims of existing projects, along with those of new undertakings.

Agricultural economics, home economics, and rural sociology are among the things which may be done under the new Act, and the emphasis laid on these has sometimes given the misleading idea that it is designed primarily for those purposes. On account of lack of funds the stations have been prevented from going into these fields extensively in the past. The urgent demand for such investigations was undoubtedly a compelling force behind the Purnell Bill, and one of the largest problems regarding the organization and conduct of investigations under the Act will relate to what it is proper, desirable, and feasible to do in these lines. The manner in which such investigation may be inaugurated and carried on, and the provision of leadership, may well be the subject of conference.

Economic questions have assumed large importance and shown the need for much more extensive data and investigation than exist. The opportunity is here offered for a coordination of effort which will be more effective than independent, fragmentary studies of questions which are broad in their nature

and applications.

As already mentioned, there is considerable work under way which bears quite directly on the field of home economics. A steady expansion of such investigation will be looked for. In this, as in other lines, the problem is the natural unit of organizing such inquiries, but sympathetic consideration for the needs of that department and the importance of developing research within its organization is to be anticipated.

Social conditions in the open country have changed very materially with the passing away of pioneer life, the tremendous growth of cities with their many material, educational, and recreational advantages, and the spread into the rural communities of auto transportation, telephones, free postal delivery, and now the radio. The isolation of farm life is being broken down to a remarkable extent, and this is creating a great variety of new social problems needing study and interpretation. Social studies are going on in ever increasing measure in urban communities, and already have led to some important and useful results. Some interesting attempts have been made to conduct similar studies on the problems of country life, but thus far these studies have been too limited and scattered to give much more than a hint as to possibilities. The Purnell Act is so worded as to permit the enlargement of investigations in this field of rural sociology.

In economic and social studies the statistical method will often have place, but it will be important to make a distinction between the collection of statistics for general purposes, and the accumulation and interpretation of statistical data pertaining to particular problems. The Federal Government and the States have their agencies for the collection of statistics of interest to agriculture, sometimes on a large scale. These statistics may be help-

ful in connection with the type of investigations appropriate under the new Act; but the experiment stations are not statistical bureaus and discrimination will naturally be made between work belonging to other agencies and that in which statistics are employed as an aid in investigation.

Much the same applies to surveys. These will frequently be needed, particularly in the preliminary stages, to show the status of a question or conditions pertaining to it. They may furnish a starting-point for more definite inquiry, the gathering of facts for use in research rather than as final ends.

The administration of the stations will form a highly important feature in determining the success of the new Act. The idea that research does not call for capable administrative attention is not borne out by the experience of the past twenty-five years. This is particularly the case when special ends are in view, and when the staffs are made up of people of varying ability in the field of research and having other duties. There has been great diversity in the amount and character of station administration, ranging from a rather small minimum confined to quite general matters, to a sufficiently close supervision to judge of the progress and the needs in particular lines. to determine causes of unsatisfactory progress, and to give careful consideration to the skillful planning of new projects.

The administrative situation has changed with this large expansion. It will not suffice for it to be casual; it will need to be aggressive, sympathetic, and discriminating. With the contemplated expansion of the field may well go a thorough survey of the work in hand, with a view to the possible redirection of some of the types of work long in progress. The size of the personnel will grow, and new members will need to be selected and started on their work with care. These things will call more than ever for an officer

of experience in research, with time to study the work and the field of the station, to counsel with and promote the efforts of his staff, and to maintain the necessary contacts within the college and outside of it.

Along with the strengthening of administration, the aim will naturally be to promote by all feasible means the conditions which make for steady and substantial progress in investigation. This applies not only to the essentials in the way of facilities, but to sufficient freedom of time, opportunity to work and to study without frequent disturbance, and arrangements which will insure needed aid from other departments.

It is still not infrequent to find cases in which the research projects are suffering because of unexpected pressure for teaching. This may mean that an individual has been entrusted with too many projects, or it may mean that insufficient provision has been made for the teaching. A director recently transmitted the report of an Adams project leader in which the latter bemoaned the excessive amount of teaching he had had to cover, confessed that it had been practically impossible to carry on what should be done, and expressed the hope that another year the teaching schedule would not interfere with his prosecuting the project energetically. In another case the head of a department dropped out and his assistant was obliged to take over the teaching in addition to his own, with the result that he could give only incidental attention to his station work. Financial conditions in the college resulted in delaying for some time the attempt to get a successor. In still another instance, a project leader had 26 hours of laboratory and 8 hours of lectures a week in the part of the year when his project should have been most active. These and similar cases may be regarded as exceptional, but they are sufficiently frequent to suggest an unfavorable symptom. Workers with only a small

fraction of their time for research, one-fourth or even one-sixth, continue to be relied upon much too far. This makes the work incidental and secondary.

It is not infrequent either to find lack of necessary collaboration in matters which are important. For example, a professor of horticulture reported that soil samples from a series of his plats had been awaiting analysis for three years. Perhaps such analyses were not needed, but that point should have been settled long ago. In some cases investigations are one-sided or quite partial because they are undertaken without reference to the whole question but only a subject-matter aspect of it. It will be realized that the station is more than an aggregation of independent departments held together by a name. It is an organization, and it needs to present a strongly organized front in its investigation. The approach to many problems is from more than one angle, and in many cases there may be more than one cause. This emphasizes the increased effectiveness of cooperative or coordinated effort.

It is vitally important that the coming of the Purnell Act shall mean more than additional funds and an enlarged field. Some new projects will be needed, but perhaps the total number will not be greatly enlarged permanently. As pointed out, it isn't the range of work or the number of projects that counts most; it is the nature of the work and how far it goes. Obviously projects should not stop with partial answers, or results be left up in the air. They ought to be reduced to practice and not dropped until they stand the test. A project may be active without being progressive; that's why some are so long in coming to a doubtful end.

Everyone realizes what a large amount of experimenting has been and still is ineffective. This always will be the case to some extent, but we can ill afford to let it go undetected. A vast amount of benefit to agricul-

ture has come from the simpler forms of experimenting in the earlier stages of our work, and great strides now are being made in thorough-going investigation of the most intensive type. These things are the glory of the American stations and the confident assurance for the future. At the beginning of this new era then, taking account of stock, sifting projects critically as to their progress and outlook, and setting high standards of expectation will be excellent preparation for using the new fund effectively.

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